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in my calculations. On page 65 it is stated that the actual position of the nucleus was 18′·6 in advance of the calculated position. Soon after the publication of my article, I received from Prof. Bond a letter bearing date July 10, 1861, of which the following is an extract: 'I have just received the *American Journal* for July, and, in looking over your interesting article on the comet of Donati, see at once that in the chart on p. 64 you have taken 7^h Gr. for the time to which it corresponds, instead of $8^h\ 30^m$ Gr., for which it was actually projected.'

"The error was, I have no doubt, an inadvertence on my own part in the copy sent you. The correction will be easily made, as the tail moved, through nearly its entire length, on that day with a daily motion of $4\frac{1}{10}°$ normal to its curve. The nucleus itself 4°. Making the correction referred to the calculated and actual positions of the nucleus differ only $3\frac{1}{2}'$."

Statement of Views respecting the Sidereal Universe. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

It appears to me desirable to conclude my discussion of the relations presented by the Sidereal Universe—so far, at least, as these pages are concerned—by indicating briefly but distinctly the points in which my views differ from the views advanced by those who have preceded me in the discussion of this subject.

I must premise that Sir John Herschel, the late Earl of Rosse, Whewell, and Herbert Spencer have advocated more or less decidedly some of the opinions which I have been led independently to enunciate. Regarded as a whole, however, my views are original, and several of the points to which I draw attention were first noted, I believe, by myself. If not, I would desire to be corrected, so as not to present as new views opinions which have already been advocated by others.

In the first place, then, I lay down as a fundamental rule that no *hypothesis* as to star-magnitudes or star-distribution can safely be adopted as a basis of research. In ordinary subjects of inquiry, it is well to have working hypotheses, varying perhaps as we proceed, but serving conveniently for the co-ordination of observations. But in researches into the constitution of the Stellar Universe, we must not adopt any hypothesis until observations sufficiently numerous and extensive have indicated its justice.*

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* It is rather singular that my inquiries, *the first systematic inquiries ever based on observations as they stand*, *without assumption of any sort*, should have been so misunderstood by several as to be described as theoretical by comparison with such inquiries as the earlier work of Sir W. Herschel, based on assumptions

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It was owing to his failure to recognise this principle at the beginning of his work (though, I need hardly say, it is to his work that I owe the main evidence in favour of the principle) that Sir W. Herschel formed the stratum theory of the sidereal system, which French and English text-books of astronomy persist in describing as a demonstrated theory, though Herschel himself definitively abandoned it. It will be well to consider this case fully, partly because no opportunity must be lost to indicate the mistaken nature of the text-book accounts of Herschel's work, and partly because of the intrinsic importance of the discovery which Herschel really effected.

It is known that, in 1784, Sir W. Herschel, who then supposed that our sidereal system is continuous within its limits, and consists of stars strewn (numerically, not in respect of real magnitude) with a certain general uniformity throughout the scheme, suggested his plan of star-gauging. It is clear that, adopting these hypotheses, a telescope powerful enough to penetrate to the limits of the system in all directions, could be employed to determine the shape of the system. For the number of stars in any given direction (within a given field) would afford a measure of the extension of the system in that direction.

And manifestly, if we add to the two hypotheses just stated this further hypothesis—that the sidereal system has some tolerably regular figure, as spheroidal, disk-like (circular or oval), cloven discoid, or the like, then a considerable number of scattered gaugings would afford sufficiently accurate general evidence as to the true nature of such figure.

It was from the combination of these hypotheses, and from a moderate number of gaugings, that, in 1785, Sir W. Herschel derived the impression that the sidereal system is shaped like a cloven flat disc, having its greatest extension towards the Milky Way, all whose stars form part and parcel of the system, and conform (according to the views of 1785) to the hypotheses adopted by Herschel.

In 1802, Herschel definitely indicated his abandonment of the hypothesis of generally equable distribution, and necessarily with that hypothesis he gave up the cloven-stratum theory of the sidereal universe, and the supposition that the stars of the Milky Way are distributed like those immediately surrounding us. The following passages from the paper of 1802 will serve to indicate Herschel's change of views even to those not well acquainted with his earlier papers; but the real significance of these passages will only be appreciated by those who have thoroughly mastered his whole series of papers (*which, judging from my own experience, implies the careful perusal of the whole series at least*

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which were not only questionable in themselves, but were rejected eventually by Herschel himself, or with such investigations as those of Struve, based, as Encke long since pointed out, on assumption upon assumption! Those who are acquainted with the literature of the subject have, however, avoided this mistake.

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three successive times, and the study of certain portions in the same way in which one would study some difficult mathematical proposition):—"The stars we consider as insulated," he writes, in 1802, "are also surrounded by a magnficent collection of innumerable stars, called the Milky Way, which must occasion a very powerful balance of opposite attractions to hold the intermediate stars in a state of rest. For though our Sun, and all the stars we see, may truly be said to be in the plane of the Milky Way, yet I am now convinced, by a long inspection and continued examination of it, that the Milky Way itself consists of stars very differently scattered from those which are immediately about us. . . . . On a very slight examination it will appear that this immense starry aggregation is by no means uniform. The stars of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments. . . . . The milky appearances deserve the name of clustering collections, as they are certainly much brighter about the middle and fainter near the undefined borders. For in my sweeps of the heavens it has been fully ascertained that the brightness of the Milky Way arises only from stars; and that their compression increases in proportion to the brightness of the Milky Way. . . . . We may indeed partly ascribe the increase, both of brightness and compression" (in these clustering regions), "to a greater depth of the space which contains these stars; but this will equally tend to show their clustering condition; for, since the increase of brightness is gradual, *the space containing the clustering stars* MUST TEND TO A SPHERICAL FORM, *if the gradual increase of the stars is to be explained by the situation of the stars.*"

Nine years of labour passed before Herschel again dealt with general considerations respecting the sidereal universe.* I will quote one passage from the paper of 1811, which deserves to be carefully studied by some who have considered it a sort of high treason in me to question the soundness of the cloven-stratum theory of the sidereal universe:—"I must freely confess," he says, "that by continuing my sweeps of the heavens, my opinions of the arrangement of the stars and their magnitudes, and of some other particulars, has undergone a gradual change; and indeed

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* Too commonly those who write of his views forget these long intervals of labour. We have now followed Herschel to the year 1811, *twenty-six years* from the time when the cloven-stratum theory was enunciated; yet I have seen sentences from the paper of 1811 quoted in immediate connexion with remarks from the papers of 1784 and 1785. Even at our own meetings I have heard assertions as to Herschel's views based on the combination of opinions in the papers of 1817 and 1818 with those expressed a third of a century earlier; while Struve, who advocated the later opinions of Herschel as against the views of 1784 and 1785, has been quoted in terms implying perfect agreement between his views and those expressed in the whole series of Herschel's papers. This is as though Laplace should be quoted as holding opinions identical with those which Kepler entertained when he broached to Galileo his ideas respecting the relations of the planetary distances to the dimensions of the regular solids.

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when the novelty of the subject is considered, we cannot be surprised that many things formerly taken for granted should, on examination, prove to be different from what they were generally but incautiously supposed to be. For instance, an equal scattering of the stars may be admitted in certain calculations; but *when we examine the Milky Way*, or the closely compressed clusters of stars, of which my catalogues have recorded so many instances, this supposed equality of scattering must be given up. We may also have supposed nebulæ to be no other than clusters of stars disguised by their very great distance; but a larger experience and a better acquaintance with the nature of nebulæ will not allow a general admission of such a principle, although undoubtedly a cluster of stars may assume a nebulous appearance when it is too remote for us to discern the stars of which it is composed."

Another hypothesis adopted by Herschel, though it was not eventually abandoned like those on which his star-gauging was based, equally illustrates how unsafe is the adoption of hypotheses in inquiries directed to the subject of the constitution of the universe. I refer to that method of gauging which he based on the supposed space-penetrating powers of his telescopes. This method was suggested in 1817, when Herschel was in his seventy-eighth year, and some results of its application were discussed in 1818. It appears to me that, while the papers of 1817 and 1818 afford unquestionable evidence of *power*, they show equally clear traces of the failing elasticity of his wonderful mind. Having devised a working hypothesis, he works straight on with a diligence and effect worthy of his best days; but he does not look around for tests as he did in the days when the first method of star-gauging was in use by him. Or rather (for, in his earlier years, he did not so much search for tests as recognise those which were in effect applied as he worked), we do not find him quick to notice, in 1817 and 1818, how his results negatived his hypothesis.

I conceive that no one who considers the real significance of Herschel's results, in 1817 and 1818, can fail to admit that they are utterly inadmissible, and that they therefore show the theory on which they are based to be altogether untenable. Herschel had taken resolvability as a test of distance. But he found regions which, while partially resolvable with his lowest powers, were not wholly resolvable with his highest. These limited regions of the heavens indicated, according to his hypothesis, long stellar projections turned directly towards the Sun, appreciably cylindrical but really conical, with the Sun at their vertex —*a monstrous supposition, and one which Sir W. Herschel would certainly have rejected in his earlier years.* It was by reasoning justly on precisely such results that Sir John Herschel was led to regard the Magellanic Clouds as roughly spherical. Sir W. Herschel had arrived, as we have seen, at a precisely similar conclusion respecting the figure of the clustering aggre-

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gations of the Milky Way. Yet it was to some of these very aggregations that he now applied the new gauging test, with results thus irreconcilable with those he had before obtained. It was by applying this new method that he arrived at the conclusions on which Struve afterwards so strongly insisted, that parts of the sidereal system are absolutely unfathomable. Yet his former and sound principle of interpretation, and the principle which Sir John Herschel, Whewell, and Herbert Spencer applied to the Magellanic Clouds, show that where *Herschel thought he was penetrating to the extreme limits of the sidereal system, he was in reality only analysing more and more searchingly an aggregation in which many orders of stars were mixed up. What he failed to do was, not as he supposed to sound the Galaxy, but to recognise as separate stars the minutest orders of orbs included within such aggregations.*

Struve, strangely enough, supposed that he himself had either made no assumptions which could fairly be regarded as such, or else none but the safest assumptions. Forbes and Encke more justly weighed Struve's researches. His assumptions, indeed, were the most daring which perhaps have ever been applied to the subject we are upon. Encke enumerates five assumptions, all of them questionable. I shall note only two, both occurring in the process by which he constructed his famous *Section* of the sidereal system, about which some have written in terms which would be exaggerated if applied to the labours of the elder or the younger Herschel.* Having counted (or rather obtained directly from Weisse's numbered catalogue) the numbers of stars, down to the ninth magnitude, in the different hours of an equatorial zone *thirty degrees wide* (from + 15° to − 15°), he first assumes that the distribution there indicated may be regarded as true *for the Equator itself*; and then, secondly, he assumes that the stars towards each hour of the equator may be distributed over the sector formed by radial lines from the Sun to the extremities of that hour-arc, according to certain assumptions as to the relative distances of the various orders of star-magnitude. The combination of the twenty-four sectors constructed on these assumptions, which are not only not supported by evidence, but strongly opposed by all the evidence thus far obtained, forms Struve's equatorial star-disk. On this insecure evidence, and on a singular mistake as to the meaning of an expression employed by the elder Herschel,† Struve based his famous hypothesis of the extinction

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* I wish it to be carefully noted that, throughout my inquiries into the constitution of the heavens, I have not said one word which has indicated other than a just appreciation of the value of the Herschels' *labours*.

† Herschel wrote, that "*when* he could not resolve the Milky Way it was because it is unfathomable," meaning clearly that certain parts of the Milky Way lay beyond the power of his strongest telescopes. Struve probably in making extracts in German, wrote "wenn" for "when;" at any rate, in the French of his *Etudes d'Astronomie Stellaire*, we find the word "si" for the English "when," and thus the whole meaning of the sentence is modified.

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of light, his analysis of the proper motion of the Sun in magnitude, and other results equally interesting, and, in my judgment, equally unreliable.*

Sir John Herschel, so far as his own inquiries (as distinguished from those which he carried out in pursuance of his father's plans) were concerned, carefully avoided the adoption of any hypothesis, basing his opinions on observed facts solely. I must note, however, that he does not show the same acquaintance with his father's series of papers as Struve appears to have obtained. I think this is not altogether to be wondered at. Sir John Herschel took too large a part in original astronomical investigations to have leisure for that thorough investigation and analysis of his father's series of papers by which alone their real bearing can be ascertained. Be this as it may, the fact is patent that whereas Sir W. Herschel abandoned the method of gauging which he had adopted in 1784, Sir John Herschel applied this method to the Southern heavens. It is not wonderful that he obtained a result not according with that which his father had obtained in 1785, for it is the property of erroneous methods to lead to incongruous results. It was thus that the cloven flat-ring theory was adopted by Sir John Herschel in preference to the cloven flat-disk theory.

But Sir J. Herschel's discussion of the peculiarities which he had recognised in the distribution of stars, his analysis of the features of the Milky Way, and his treatment of the evidence derived from the Magellanic Clouds, are characterised by sound and clear-sighted reasoning. We owe to him, in fact, the initiation of those processes and methods of inquiry which have recently been applied in a more systematic manner.

Passing to my own treatment of the subject, I remark, first, that it has seemed to me very necessary to substitute for statistical research, at least in the beginning of inquiries into the laws of stellar distribution, some process not requiring preliminary hypotheses. I can conceive no general statistical process absolutely free from hypothetical considerations. Statistics can be satisfactorily applied to inquiries suggested by other and less deceptive processes; but at the beginning we cannot *count*, except on accordance with some pre-arranged plan, and such plan must necessarily be based on hypothesis. We see this in Struve's researches. He counted the number of stars in given hours of right ascension; but the result was meaningless, except on the assumption that the distribution of stars over a given hour, and between declination-circles + 15° and − 15°, possessed a certain significance. We see, in fact, that he regarded the richness of

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* I write this with some confidence. Sir J. Herschel has shown the weakness of the reasoning on which Struve based the theory of the extinction of light. As to the rate of the Sun's proper motion, I showed long ago the probability that this rate exceeds that assigned to it by Struve; and Dr. Huggins's recent researches into stellar motions of recess and approach may be regarded as conclusive on the subject.

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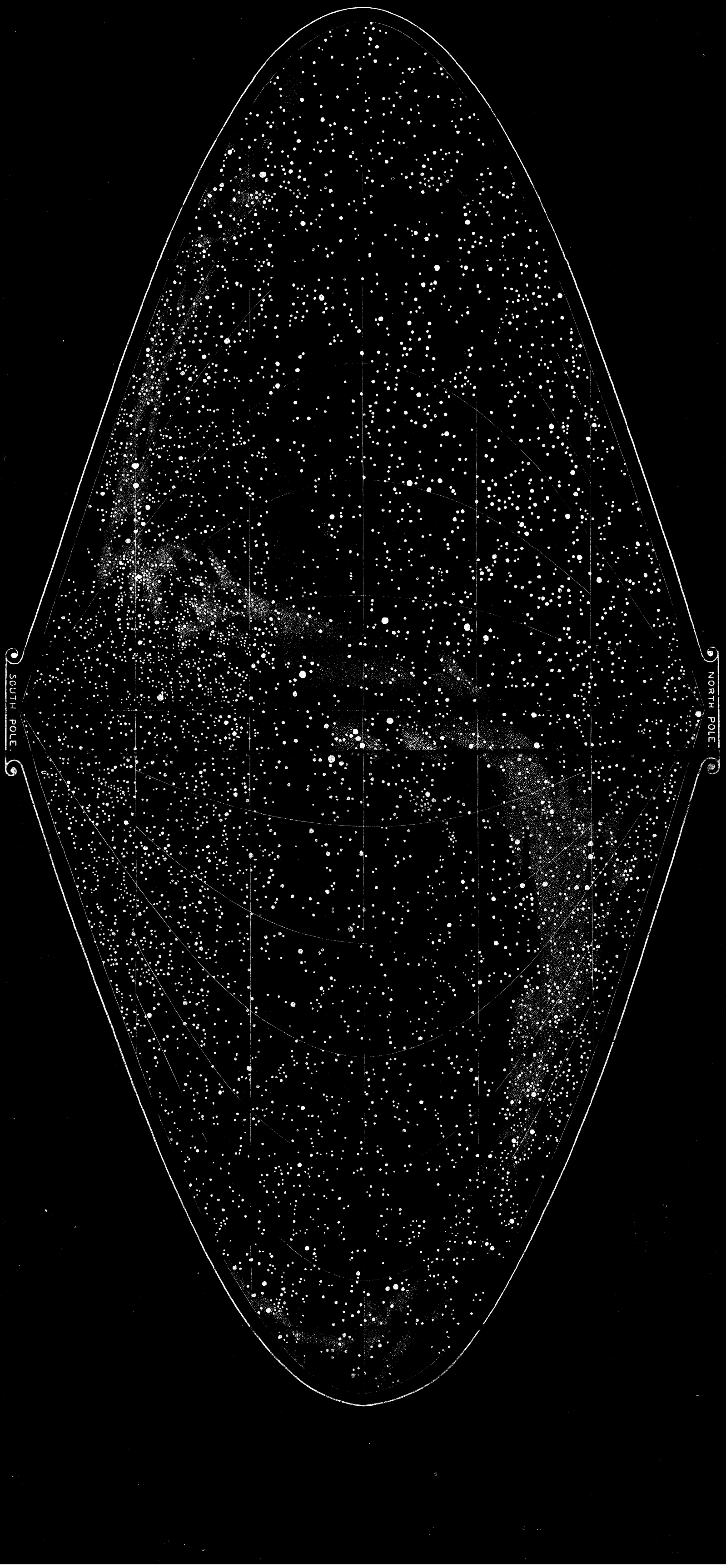

THE STAR-SPHERE ON FLAMSTEED'S PROJECTION (EQUAL SURFACE) SHEWING ALL THE STARS VISIBLE TO THE NAKED EYE.

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distribution around the Equator itself as indicated by the richness of star-distribution over areas thirty degrees in declination by fifteen degrees in right ascension; and unless that assumption, or some like assumption, were made, the mere numbers he obtained could not, so far as I can see, have any significance at all.

But then there is the risk in such a case that we may be misled in giving significance to statistical results by means of some hypothesis,—the result only becoming significant by being made erroneous.

Charting, so long as it is isographic, seems open to no such objection. We plot down stars, or nebulæ, or variables, or red stars, or double stars, as the case may be, and accept the result, whatever it may be, certain at any rate that we shall not be taught anything erroneous, though the charting may not lead to any result of importance. Whatever there is to learn, we shall probably learn from a well-constructed chart; and we may learn much more than had been hoped when the chart was commenced, or else—and this has repeatedly been my own experience—a chart, while not in itself teaching any noteworthy lesson, will suggest either statistical research or some new process of charting by which important new knowledge may be obtained.

From my equal-surface charts of the brighter orders of stars, I infer—I think I may even say I have demonstrated—that there is a much greater range of difference in real star magnitudes than had been supposed. Unless the laws of probability are to be questioned, those lucid stars which gather richly over the area of the Milky Way are really immersed among the clustering aggregations of smaller stars which produce the milky light of the Galaxy. In this case those brighter stars must be not only *much* larger, but many hundreds of times larger than the fainter stars of the Galaxy.

We have here the first step towards just views of the constitution of the Milky Way, or rather the next step beyond the great, but little noticed, discovery of Sir W. Herschel's, that the bright clouds of the Milky Way are for the most part spherical clusters of stars. The excess in the number of lucid stars in the Milky Way proves that the stars of our constellations are among the leading orbs of these spherical aggregations. The accompanying charts, viz. the chart of stars and nebulæ, and the wood-engraving, serve to exhibit other peculiarities illustrated by the mapping of stars visible to the naked eye. Little is required by way of explanation of any of the charts, which are, as all such charts should be, self-explanatory. It is only necessary to mention that the large wood-engraving exhibits the whole heavens on Flamsteed's projection (which is an equal-surface one); and although in parts the distortion is monstrous, yet as the distribution is always truly represented, the great purpose of star-mapping, so far as concerns our subject, is fully served. However, it would be a useful exercise to those having sufficient leisure, to make other similar

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charts on the same projection, but taking other meridians for the central line. Charts on the same projection, but on a larger scale, and including the nebulæ, as in the lithographic engraving, would also be highly instructive. I can, however, only indicate at present the desirability of such charts, my time being too fully occupied with other matters to enable me to carry out all the mapping processes which I should like to see applied. Before long, should I not be anticipated, while carrying out the processes of star-gauging which I have already commenced, I shall probably also obtain opportunities for constructing a complete series of charts illustrative of the laws of stellar and nebular distribution; but for the present those already drawn may be regarded as sufficient. Indeed it is not to be expected that others *applied to the same objects*, would reveal new relations of importance, though strengthening and elucidating the evidence given by those already drawn.

Charts including stars down to the seventh and eighth orders, yet remain to be constructed. I am not acquainted with any suitable materials. For the purpose of true star-gauging it is essential that the same uniform scale of star-magnitudes should be used; and as yet no complete catalogue or series of charts in which the stars of the seventh and eighth magnitudes have been estimated according to a uniform scale, are accessible, or at least known to me. I may remark, indeed, that charts alone would serve my purpose, since the labour of constructing equal-surface maps from complete catalogues, including stars down to the eighth magnitude, would be enormous, and though the results would be interesting, they would probably scarcely repay the labour.

The existence of Argelander's series of charts of stars visible in the Northern heavens with a telescope 2¾ inches in aperture alone rendered my next step—the construction of an equal-surface chart containing all such stars—a feasible, though not altogether an easy one. I must remark, that the charts of Argelander's series cannot be regarded as quite exactly representing the distribution of stars over the whole of the Northern heavens. Anyone who will test them systematically with the telescope, will soon recognise the fact that many stars well within the range of a telescope 2¾ inches in aperture have been omitted. This, of course, was to be expected. But it is remarkable that in many cases stars have been noted which are certainly not ordinarily to be seen with Argelander's selected small telescopic power.

Nevertheless, regarded as a whole, the series of charts constructed under Argelander's superintendence represents fairly the distribution of stars down to the eleventh magnitude of Sir J. Herschel's scale. The inexactnesses relate to minutiæ, not to the broad features which can alone be dealt with in the present stage of the inquiry.

I must remark here, that although in my equal-surface chart

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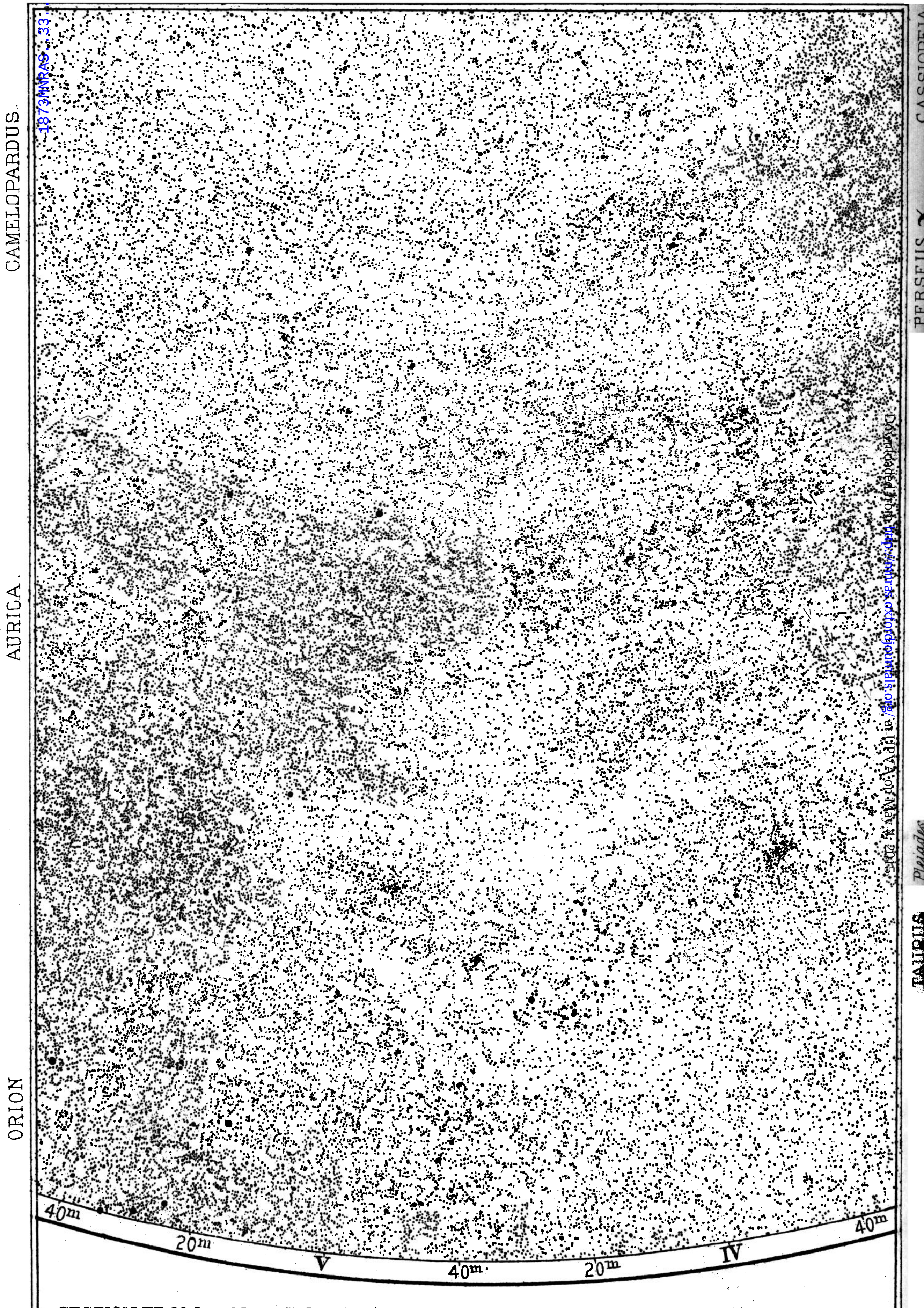

SECTION FROM A CHART OF 324,198 STARS ON AN EQUAL SURFACE PROJECTION

R. A. Proctor del.

M&N. Hanhart, photo lith.

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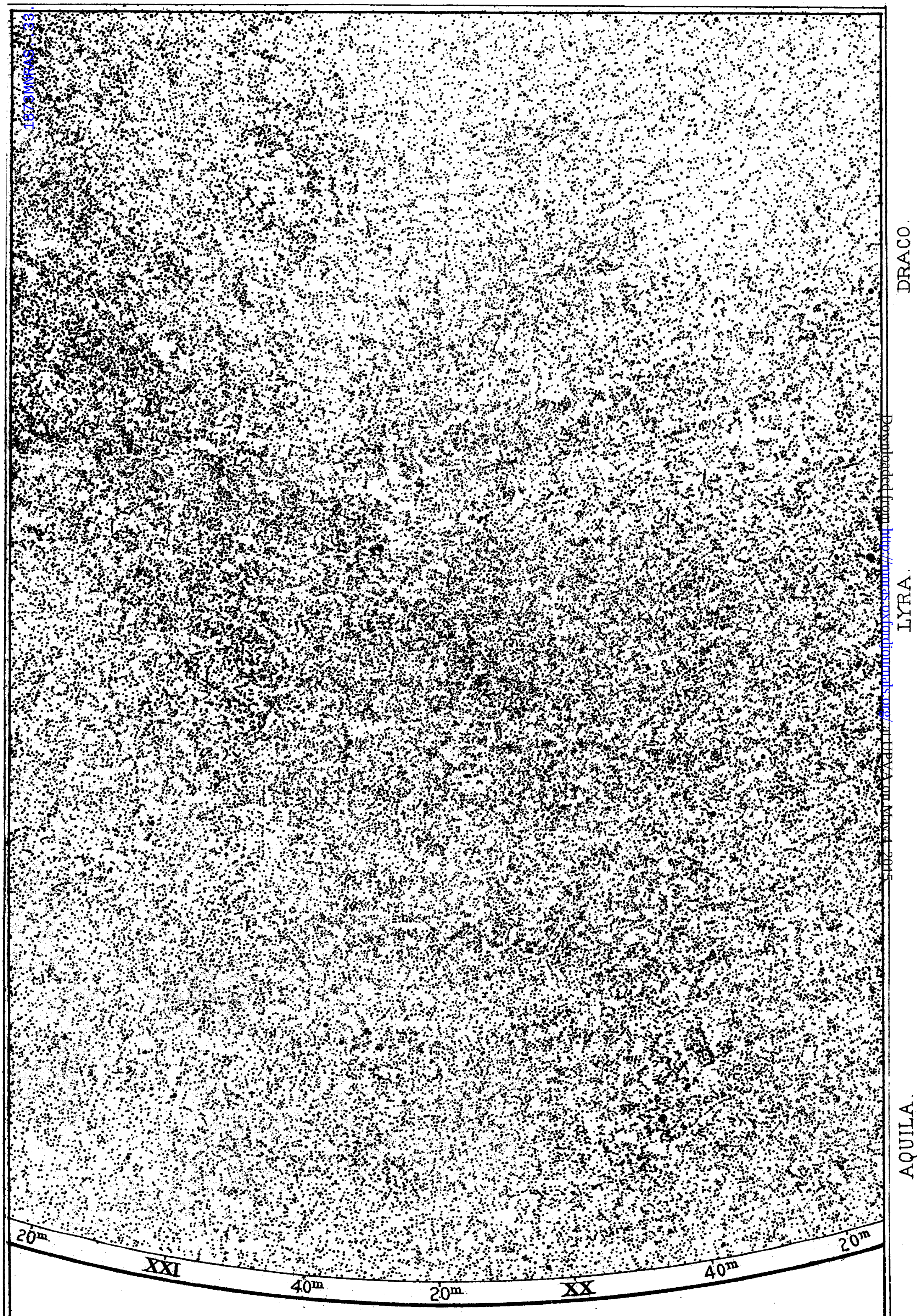

SECTION FROM A CHART OF 324,198 STARS ON AN EQUAL SURFACE PROJECTION.

R. A. Proctor del

M&N Hanhart photo lith.

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including Argelander's series, I have marked down all the stars, as nearly as the eye could judge, in their true place within the spaces (one degree each way in R.A. and Decl.), to which they severally belong, I have by no means done this with the intention of constructing a chart which could be used for reference to individual stars. In point of fact, I wanted only to ascertain the general distribution of the stars, and my purpose would have been equally, or nearly, as well fulfilled if in each space I had marked down *at random* the proper *number* of stars. But, strange though it may seem at a first view, it was easier for me to make an exact transcript for each space than merely to jot down so many stars. The reason is, that by copying from each space I had at once a means of distinguishing one space from another in my chart as compared with the original. Nothing could have been more baffling than the attempt to preserve merely the numerical distribution; and of course the result was somewhat more accurate, even as regards the indication of laws of aggregation.

The significance of the results obtained by this second step—that is, the step from stars visible to the naked eye, to stars visible with 2¾ inches of aperture—can only be appreciated by those who have carefully followed, and thoroughly mastered, the reasoning of the elder and younger Herschel, and of the elder Struve. I assert, without the slightest fear of contradiction by any possessing such knowledge, that *the broad teaching of the equal-surface chart of* 324,000 *stars disposes finally of all theories of the constitution of the sidereal universe which had previously been enunciated.* The chart does not definitively indicate a new theory—rather it suggests the idea that the constitution of the sidereal universe is too complex to be at present ascertained. But it completely negatives (i), the stratum theory (even in the modified form apparently retained by Sir W. Herschel); (ii), the flat-ring theory of Sir John Herschel; and (iii) the infinitely extended stratum theory, with condensation towards the mean plane, which Struve adopted.

The force of the evidence derived from the chart (specimens from which are herewith submitted) consists in the position which the gauging power of Argelander (for it is to his gauging power that I alone appeal) bears to naked eye vision on the one hand, and to the powers used by the elder Herschel on the other. Powers intermediate to Argelander's and Herschel's may give important evidence, and doubtless will, when applied in accordance with the plan I have elsewhere suggested; but such evidence cannot have the value which resides in Argelander's work. A much lower telescopic power than Argelander's would bring the range too near that of ordinary vision; a much higher power would bring the range too near that of the Herschelian gauging telescopes. It is the intermediate character of Argelander's telescopic survey which constitutes its real value.

This I propose briefly to indicate.

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The eye shows the great Milky Way stream, with its irregularities, branching extensions, nodules, and other peculiarities. The eye also indicates (in combination with suitable processes of charting and enumeration) the gathering of lucid stars on the Milky Way. Such is the evidence at one extremity of the scale. At the other we have the result of the Herschelian star-gauges, and especially of those gauges, not tabulated, which both the Herschels applied to rich Milky Way regions; and we learn from it, according to the words I have already quoted, that in Herschel's sweeps of the heavens "it has been fully ascertained that the brightness of the Milky Way arises only from stars, and that their compression increases according to the brightness of the Milky Way." This relates, as we know, to the stars brought into view by his great telescopes. Nothing, then, can be more interesting and important in connexion with the subject of the sidereal universe than the question whether the naked-eye brightness, and the gathering of naked-eye stars on the one hand, and the great richness of stars of the faintest orders reached by our telescopes on the other, are accompanied by an exceptional richness of stars brought into view by an intermediate power,—far enough removed from naked-eye vision on the one hand to give those large numbers which raise probabilities into certainties, and far enough removed from Herschel's space-penetrating power on the other hand to ensure a real selection of larger stars from among the practically infinite numbers of minute stars revealed to his scrutiny. This would in any case be important, as Struve long since demonstrated, and as Herschel himself clearly recognised, as affording general evidence of the laws of stellar condensation; but the comparison assumes a far greater importance when we remember what Herschel demonstrated as to the generally insulated nature of those aggregations which lie along the Milky Way streams. Let the meaning of his result be rightly appreciated; let it be clearly remembered *that he had in effect proved the Milky Way to consist in the main of real clouds of stars arranged in real streams in space*, and we see at once that to prove that these clouds are richer, both in minute stars and in relatively bright stars, than surrounding regions, is to prove the existence, within each of them, of widely different orders of real star magnitude. When we add the consideration that the Herschels, with their most powerful telescopes, were in some cases unable to resolve the richer regions of the Milky Way, we begin to recognise something of the complexity and variety of structure existing where uniformity of structure and variety of distance had been imagined.

It is obvious that what has thus been recognised respecting the system of stars is in perfect accordance with what has been ascertained respecting the distribution and nature of the nebulæ. So long as we assumed the stellar system to be of a tolerably uniform nature throughout its extent, there was some difficulty in admitting as probable the existence within that

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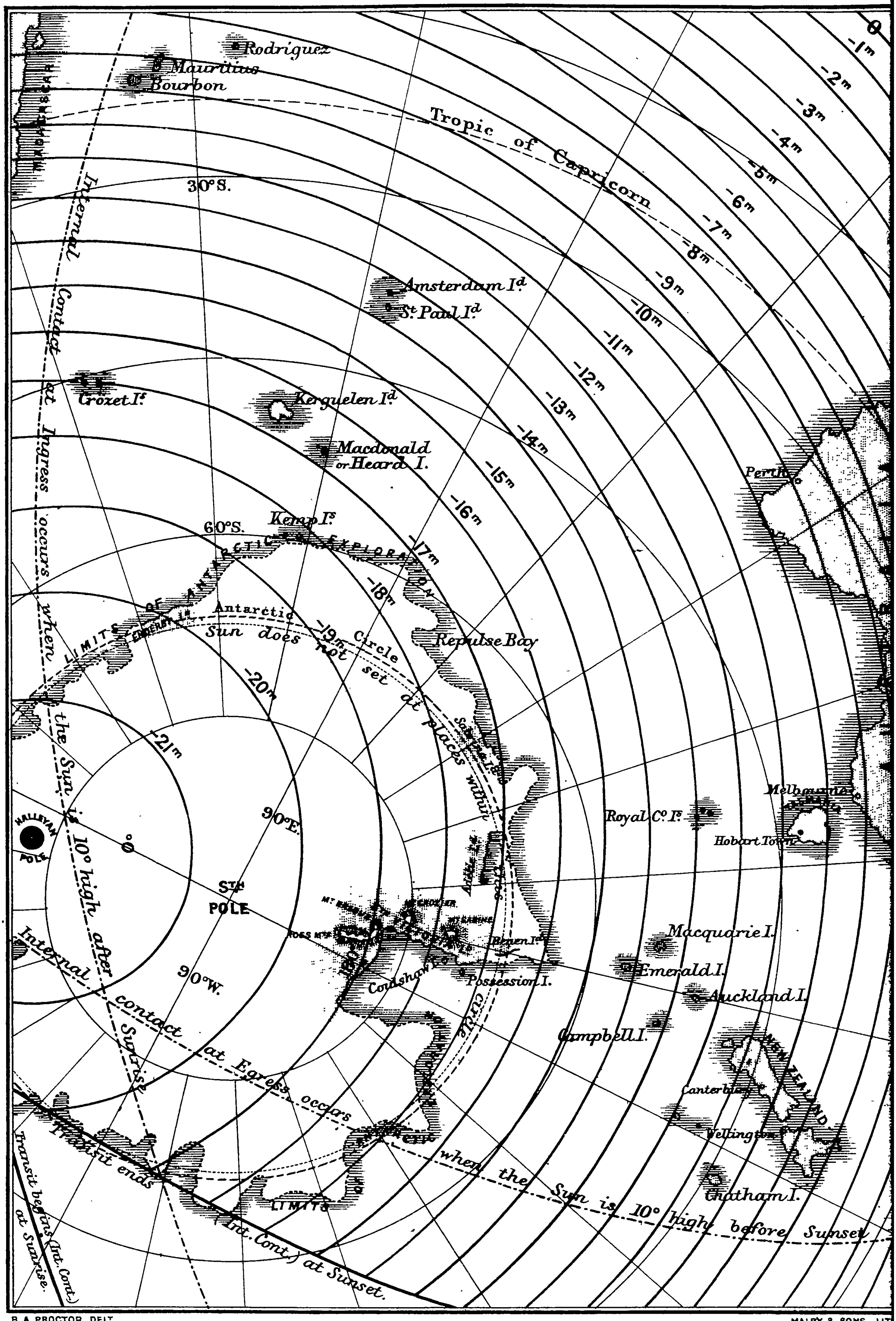
TRANSIT OF VENUS IN 1874.

CHART OF THE ANTARCTIC AND SUBANTARCTIC REGIONS WHERE THE DURATION OF THE TRANSIT WILL BE SHORTENED.



R. A. PROCTOR, DELT. MALBY & SONS, LITH.

-18m Signifies that the duration is 18m less than the Mean at all points along the curve so marked.

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system of structures so distinct as the nebulæ, whether resolvable, irresolvable, or gaseous. Even the evidence afforded by the Magellanic Clouds as to the nature of all the orders of nebulæ, convincing though that evidence really is, did not impress the truth on the minds of those who yet could point to no flaw in the reasoning. The relations indicated by my equal-surface charts of the nebulæ, though equally satisfactory as evidence (besides being independent and therefore the more convincing), were similarly inoperative; and I think the beautiful, and in my judgment most valuable maps by Mr. Waters which appear in the present number, would not carry conviction of themselves.

But I submit that when we perceive the variety of stellar structure and aggregation, as well within the Milky Way as in extra-galactic regions, the evidence respecting nebulæ is brought into such perfect accordance with the evidence respecting stars that its real significance can no longer be misapprehended.

Here, moreover, the common evidence given by stars and nebulæ (agreeing also perfectly with that given by the Magellanic Clouds) is strengthened by the evidence derived from *details* of structure in the Milky Way, on its borders, and outside of it. I cannot here do more than allude to the nature of this evidence. I would note, then, that the balance of attractions within the Milky Way which Sir W. Herschel pointed out, suggests a reason as well for the paucity of irresolvable nebulæ within its bounds, as for the number of irregular clusterings and ordinary clusters contained within it. As Sir W. Herschel pointed out, the clusters in and near the Milky Way may be looked upon as so many portions of the great mass drawn together by the action of a clustering power of which they tend to prove the existence. But this clustering power has been interfered with and checked by the balance of attractions. Outside the bounds of the Milky Way, a clustering power uncontrolled by any such cause has been at work, and to its influence may be ascribed those closely set nebular clusters which are either irresolvable or can else only be resolved with very powerful telescopes.

The gradual diminution in the number of clusters as we leave the galactic zone and the equally gradual increase in the number of irresolvable nebulæ show the reality of the connexion here indicated. There is an even more convincing though more delicate proof, in the continuous change of character of the nebulæ from the most scattered clusters of the Milky Way to the absolutely irresolvable nebulæ of extra-galactic regions. Of course the actual constitution throughout the whole heavens has in part depended on the distribution of star material, and not on position only. We see the influence of this in the charts of stars and nebulæ in *Virgo* and *Coma* which I contributed some time since to our Proceedings, and on a wider scale in Mr. Waters' beautiful charts, where we notice how, as irresolvable nebulæ become scarce, scattered clusters appear, not only on the heavens regarded as a

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whole, but in the several parts of the extra-galactic nebular system, and streams.

The evidence respecting nebulæ known to be gaseous points the same way; but it may be desirable to wait before summing up this part of the evidence until the spectroscope has been more widely and systematically applied to the nebulæ.

It is, perhaps, hardly necessary to insist on the evidence which the discussion of stellar proper motions can afford on questions relating to the structure of the universe. The general tendency of star motions, and the average magnitude of such motions would supply evidence of extreme importance towards the discrimination of stellar aggregations in particular regions of space.

And here I must point out two mistakes which have been made by some persons who have spoken of the phenomenon which I have called "star-drift."

One mistake is the confounding of this phenomenon with the star-drift due to the Sun's motion in space, which drift was first recognised by Sir W. Herschel and has since been more widely recognised by several other astronomers. This star-drift is general, but is seen only in the average of stellar motions. The star-drift which I have indicated is local, and has a different significance altogether. Before I had recognised it (in fact, before I had contributed any paper to the pages of our *Notices*), I suggested its probable existence. When I had recognised it in several regions of the heavens, as in *Gemini*, *Ursa Major*, and so on, I pointed out how my theory on the point could be tested by the spectroscope, and I selected the drift in *Ursa Major* as specially suitable for the purpose. I described the results which should follow if my theory were sound, and as is now well known my expectations were confirmed to the letter. The star-drift thus demonstrated in *Ursa Major* is a phenomenon perfectly distinct from the star-drift due to the Sun's motion. It takes place, as my maps of the stellar proper motions show, in almost exactly the reverse direction from that due to the Sun's motion. Moreover the real interest of the phenomenon consists, *not* in any evidence it gives as to the general motions of the sidereal universe, but in the fact that it marks out the stars affected by it as forming a subordinate system separated from other star-regions by starless spaces of enormous extent. Moreover the recognition of star-drift, especially if accompanied by spectroscopic evidence as to star structure, will probably become a potent means of ascertaining the architecture of various parts of the stellar universe.

The other mistake resides in the supposition that at the present stage of the inquiry it would be desirable for me to examine and discuss those observations which promise to afford more complete information than we now possess respecting the proper motions taking place within the star system. I can only deal with wide and general relations; and the charts of proper motions already constructed by me suffice for that purpose, since their construction

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has led to the general theory of star-drift which it was chiefly important to establish. Anyone who may have leisure for the work can now examine suitable star-catalogues for the recognition or correction of estimated proper motions, with the assurance that such work will reap a rich reward. But for my own part the task I have taken in hand is of another nature, and will permit of no expenditure of time on details, however interesting the subsidiary evidence which such details may afford. I wish rather to set others at work on the various parts of the wide subject of the constitution of the heavens than to enter myself on work of detail. The structure of the star-system as a whole, or at least of the portion within our scope, is the subject to which I address myself, and too minute a discussion of the subordinate details would impair rather than improve the general view which I desire to obtain. The proportions of a great structure are not favourably studied by approaching as near as possible to some particular part of it.

Here my discussion of the subject of the constitution of the stellar heavens comes to a close so far as these pages are concerned. I take the opportunity of thanking the Fellows of the Society for the patience and attention with which I have been listened to when I have described my views at our evening meetings. I trust and believe that those who have really examined the evidence I have adduced, will not have thought me dogmatic in the expression of opinions which I have based on a very thorough investigation of all the available evidence. I have been more than satisfied with the support I have already received. From Sir John Herschel in particular, who perhaps alone of all our leading astronomers has taken special interest in the subject of the constitution of the heavens, I received encouraging expressions of opinion (which will one day be published), important not only in their relation to the researches I communicated to him, but in their prospective bearing on researches which have been completed since his lamented death. For the rest, it is not to be expected, and is in fact not possible, that general attention should be drawn to a subject so far removed from ordinary astronomical studies, whether practical, observational, mathematical, or theoretical. At all times in the history of Astronomy, the number of those who have been attracted to the study of the laws of stellar distribution has been small, though strictly speaking the very name Astronomy implies such study more specially than any other now recognised subject of astronomical research. But the true student of science is content if he "fit audience find though few." The investigation and demonstration of the truth are more important than the mere numerical array of supporters: (though, indeed, if on the one hand, as I had expected, few have been attracted to a subject of great difficulty, yet on the other I have had small occasion to complain of cavil or objection). And if the results to which I have been led, or may be led hereafter, are just,

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it is a matter of indifference whether they be generally accepted at a late or early season, in my own lifetime or not till long afterwards.

The Rotation-Period of Mars. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

In the *Annalen der Sternwarte in Leiden*, for 1872, there is a series of papers on the planet *Mars* by the late Prof. Kaiser. One of the papers relates to the rotation-period of the planet, and Prof. Kaiser therein endeavours to show that his estimate of the rotation-period of *Mars* — viz. $24^h\ 37^m\ 22^s{\cdot}62$ — is preferable to mine — viz. $24^h\ 37^m\ 22^s{\cdot}735$. When I first read this paper, it appeared to me that, whether Kaiser were mistaken or not in his conclusions, a great difficulty was introduced; because he stated as the result of his calculations, that a drawing of *Mars* by Huyghens, in 1672, cannot be reconciled with the two drawings by Hooke, in 1666, on which I placed reliance, unless a mistake of two hours in the time be accounted for. Even then,— that is, after my first cursory reading of his paper,—it seemed to me that the readiest solution of the difficulty was not, as Kaiser suggested, to reject the work of one or other observer, but to divide the error between them. Yet it seemed clear to me that this would involve the supposition of very careless drawing *both* by Huyghens and by Hooke. I have been glad, however, to find, when my leisure permitted me thoroughly to examine Kaiser's work, that the greater part of this discrepancy is accounted for by a rather strange error in his reckoning. In estimating intervals between the observations in the seventeenth and nineteenth centuries he would seem to have forgotten that the years 1700 and 1800 were not leap years, and so makes the intervals systematically two days too great. As he thus gets two Martian rotations too many, and each exceeds a terrestrial day by $37^m\ 22^s{\cdot}7$, the error amounts approximately to the loss of $1^h\ 14^m\ 45^s{\cdot}4$. This reduces the discrepancy by more than one-half. Oddly enough, a second error in calendar matters has prevented Kaiser from detecting the error, as he might have been expected to do when he compared the observations of Hooke and Huyghens. For Hooke's observation, made on March 3, Old Style, he translates into March 14, New Style, whereas the real reading is March 13. Thus an error of $37\frac{1}{2}$ minutes is introduced as between Hooke's observations and Huyghens', and this raises the discrepancy between them so nearly to that which *should* exist if Kaiser's rotation-periods were correct, as to satisfy the requirements of the case.

As regards the balance of difference between the two esti-